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An Experimental Inquiry

INTO

THE MODUS OPERANDI

OF

STIMULI

UPON THE

HUMAN BODY.

BY PETER FOISSIN,

OF GEORGETOWN, SOUTH-CAROLINA; HONORARY
MEMBER OF THE PHILADELPHIA MEDICAL
AND CHEMICAL SOCIETIES.

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An Inaugural Thesis,

FOR THE DEGREE OF

DOCTOR OF MEDICINE,

SUBMITTED TO THE EXAMINATION

OF THE

REV. JOHN EWING, S. S. T. P. PROVOST;

THE

Trustees and Medical Faculty

OF THE

University of Pennsylvania.

400061

For
Mr Cleaver from his
friend the Author

*To Levi Myers, M. D. of Georgetown,
South Carolina.*

DEAR SIR,

IN inscribing to you the following pages, the product of those studies commenced under your immediate guidance, I am directed more by a desire of evincing to you, the grateful sensations created by a recollection of the polite attentions shewn me by you, during that period, than a vain wish of receiving your aid in screening this essay from the shafts of censure. Accept, then, Sir, with the best wishes, for your happiness, the sincere thanks (for your useful instructions) of

Your

Friend and Pupil,

PETER FOISSIN.

To Matthew Irvine, Physician, of Charleston, South Carolina.

DEAR SIR,

SINCE the practice of dedication has become so common, it must of course, in many instances, be esteemed as the mere creature of form. This I should fear would be the light in which the following would be held, were I not buoyed above the thought, by a consciousness, that you are convinced of the propriety of it, from a recollection of the indelible obligations which you did, and were ever ready to bestow on me, while I had the honor of being your pupil; for which you will please to suffer me thus publicly to acknowledge myself much indebted to you.

I am, dear Sir, with much esteem,

Your sincere Friend and Pupil,

PETER FOISSIN.

An Experimental Inquiry,

Éc. Éc. Éc.

IN entering on the investigation of one of the principles of an illustrious Professor*, I naturally feel that diffidence which is attached to youthful inexperience. I have selected, as the subject of my Inaugural Essay, an inquiry into the *modus operandi* of the great class of stimulating medicines upon the human body. The results of my experiments are unfavorable to the opinions of some eminent writers and physicians on this subject.

The animal body or system has been divided into seven subordinate systems, viz. 1st. The nervous system; 2d. The glandular system; 3d. The visceral system; 4th. The muscular system; 5th. The arterial system; 6th. Juices of the body; and, 7th. The absorbent system. On each of these systems, it has been supposed, certain medicines, or stimuli exert a peculiar, if

* Dr. Rush.

not an exclusive, influence. It has even been imagined, that this division of the body might serve as the foundation of an arrangement of the articles of the *materia medica*. Before I detail the experiments which I have made, I venture to ask the question, whether *any* division of the general system into smaller or subordinate systems, is entirely unexceptionable, or supported by a sufficient number of facts. The experiments which I have made, and the facts which I collected, lead me to suppose, that such a division, and every medical arrangement founded upon it, will ever be imperfect, if not illusory. If the theory which invests medicines, of different kinds, with an exclusive determination to one individual system; or, in other words, which gives to medicines a specific mode of action, were well founded, we should see the effects, which are attached to certain stimuli in the immutable relationship of cause and effect, as inseparably the result of their action as water is that of passing the electric spark through an atmosphere of oxygene and hydrogene gasses. If this theory were true, mercury would always, and conium and opium would never, salivate. We should thus be led to attach the action of mercury to the salivary glands, and that of conium and opium to some other system or part. But the cases which have come within the sphere of my own

observation, in which mercury refused to salivate, are sufficient to determine my opinion on that head; and a recent case, (in the Pennsylvania Hospital) in which conium induced ptyalism serves to evince an apetency in the system to translate an action, brought on in the general system, by any stimulus to a particular part; or, in other words, to concentrate and render local, a general action. If this theory were well-founded; that is, if stimuli possessed specific properties, why does not the matter of gonorrhea, when it has been introduced under the cuticle, exercise its specific agency, on those parts which are the common seats of its operation. Late experiments have shown, that this is not the case*. Do the matter of small pox, and the poison of the viper, exercise their specific action, when taken into the stomach? Dr. Barton, in his lectures on the *materia medica*, relates experiments with the poison of the viper which prove, that it does not; and an experiment with variolous matter proves the same thing with respect to it. Does the contagion (if such there be) of yellow-fever, always induce that disease? Were we to adopt an opinion in favor of it without observation, the prevalence of hepatitis, pneumonia, enteritis, gastritis, phrenitis and apoplexy, during

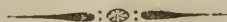
* See Dr. J. Tongue's Inaugural Thesis.

the prevalence of that disease, would be sufficient to point out the impropriety of such an opinion, and to evince the folly of judging without having previously observed. Because laudanum, in a certain instance, acted as the remote cause of yellow fever, ought we as studiously to avoid the exhibition of this medicine, as we do that house, or city, in which the contagious origin of this disease is supposed to reside? No! for in doing this we would deprive ourselves of the truly *magnum Dei donum*, in which resides, when adapted to the excitability of the system, the property of restoring sanity to the body and mind of man, when laboring under the influence of pain, disquietude of mind, or the qualms of a guilty conscience. Does the same contagion, even in three persons exposed to it, excite the same diseased action? The fact, related by Dr. Rush, of three young men who were exposed to the same miasm having each a different disease excited in their systems, may perhaps be deemed a satisfactory answer of this question. Should opium be considered as a soporific, because one of its secondary effects is sleep. I think I am justified in saying, it should not, when it is considered what was the effect of our medicine on the illustrious but unfortunate author of the *Elementa Medicinæ*. Ought we, because the odor of a certain errhines which

Dr. Cullen and his wife used in their life times, excited more vividly in their daughters the idea of their persons and manners, than any other stimulus could, to unite effect to that cause, as that of specific action? No! for errhines excite sneezing, and sometimes vomiting. Should the drinking of water, the beverage of nature, be abstained from in the summer-season, because it in that season has been in many instances the instrument of death? No! for the calls of nature would impel us to the substitution of something more deleterious. In predisposition to fever or any other species of diseased action, we have it excited by a hearty meal or generous glass of wine taken at the table of an hospitable friend, to whom and which, according to our ideas of specific action, we must impute our disease and abstain, in future, from each of them through fear of having that action always excited; in doing this, we would act like that rustic, who in consequence of having had hæmoptysis brought on by breathing pure oxygene, excluded it from the atmosphere he afterwards wished to live in, and died in the experiment; for I conceive each of them as necessary to the exercise of our mental and corporeal functions, as oxygene is to the support of animal existence. Should we never rejoice, because joy, in excess, frequently induces asphyxia, and in the Roman mother even

caused death? Should we, because fear produced death, in Saphira, the wife of Ananias, at the feet of St. Peter, chain death to fear as its immutable result? No; for fear, by stimulating the coward to fly and sometimes to fight, has been instrumental in preserving the lives of many fathers and husbands. Should the idea of excessive heat be affixed to water at the temperature of 76, because when the heat of our bodies is reduced very much below that point it conveys a sense of heat? No! For when our bodies are at their natural standard, it conveys a counter sensation. Because cold, in some instances, causes constipation and cures epistaxis, and other hæmorrhages, should we, in our *modus concatenandi*, unite these effects to this cause as its inseparable consequences? No; for the same stimulative matter in a certain nobleman, affected with an almost insuperable costiveness who, at the instance of his physician, Savonarola, got out of bed and walked over a cold marble floor, in mid winter, was excited to discharge his fæces in a very short time. I may add, that none of us are ignorant of the fact, that a cold northwest wind, blowing in our faces, will cause solution of continuity and consequent effusion of blood. The variety of result, therefore, from the action of stimuli on the animal body, created

doubts in my mind respecting the specific doctrine, and led me to institute the following experimental inquiry, which was conducted with fairness, and is detailed with candor.



SECTION I.

Of the Nervous System.

The writers on the *Materia Medica* have frequently been biased in their description of the properties of plants and other medicinal articles, by some systematic arrangement to which they have adhered. In this science, arrangements have been a frequent source of error and mismanagements in the cure of diseases. For some physicians do invariably prescribe medicines under an implicit belief that they, according to their classification, possess the power of curing certain diseases arranged in order and attached to a particular system or part upon which a certain stimulus, a medicine, has been supposed to act. This position cannot be better elucidated than by the following citation of the *modus agendi* of medicines

in yellow fever. In this disease, when the system from repletion is laboring under suffocated excitement, they, following Cullen's principles, give to this disease the name of synochus, which the Edinburgh Professor, ignorant of the true cause of prostration, says degenerates into typhus; and here, supposing typhus to have its seat in the nervous system, they begin the treatment of the disease by the exhibition of musk, assafætida, and camphor, all of which are supposed to exercise a primary influence on the nervous system. This doctrine, however, I am disposed to think, I have rendered improbable by the following experiments :

EXPERIMENT 1.

To my friend Mr. Heap, his pulse beating seventy-two strokes in a minute, I gave thirty grains of gum-assafætida, suspended in water. In five minutes, his pulse beat seventy-six; in ten, seventy-eight; in fifteen, eighty, with encreased force; in twenty, seventy-six; in twenty-five, sixty eight, with nausea and softness of pulse: in thirty, sixty-six, the nausea much increased; in thirty-five, sixty-two; he was now excited to vomit; during his efforts his pulse beat very feebly and frequently; in forty, his pulse rose to seventy;

in forty-five it was at its natural standard. Here, then, we see that this gentleman was vomited by our nervine.

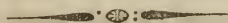
EXPERIMENT II.

Mr. Cleaver, his pulse beating sixty-eight strokes in a minute, obligingly became the subject of the following experiment: At ten o'clock he took five grains of camphor, suspended in water. In five minutes, his pulse beat seventy; in ten, seventy-two; in fifteen, seventy-six; in twenty, eighty, with increased fulness and force: in twenty-five, seventy-two, the fulness being diminished; in thirty, sixty-four; he was now persuaded to repeat the dose, which he did, with the following effects: in five minutes, his pulse beat sixty-four; in ten, seventy-two; in fifteen, eighty; in twenty, eighty-four; in twenty-five, seventy-eight; in thirty, seventy-two; in thirty-five, seventy; in forty, sixty-eight; in forty-five, sixty-eight; in fifty, sixty-eight. Mr. Cleaver informed me, that he did not, at the time, or afterwards, experience any sensible effect from this medicine.

EXPERIMENT III.

At ten o'clock, my obliging friend and fellow-graduate, Mr. Oswald, his pulse beating seventy strokes in a minute, took six grains of musk, in the form of a pill. In five minutes, his pulse beat seventy-four; in ten, seventy-six; with fulness: in fifteen, seventy-six; in twenty, seventy-six. I now repeated the dose. In five minutes, his pulse beat seventy-eight; in ten, seventy-eight, with increase of fulness; in fifteen, eighty; in twenty, seventy-four, with nausea; in twenty-five, seventy, with an increase of the nausea. The dose was again repeated; in five minutes, his pulse beat seventy-two; in ten, seventy-six; in fifteen, seventy-eight; in twenty, seventy-eight; in twenty-five, seventy-eight; in thirty, seventy-six; from which it gradually fell to seventy, at which it remained. Mr. Oswald experienced neither hilarity, nor any of those other effects, which are attributed to this stimulus; but, on the contrary, he felt a diffusion of heat throughout his system. If we prove, what I have endeavoured to render probable, by experiment, *first*, That arterial excitation is the primary result of those stimulating substances, which are supposed by most, and I believe all physicians, to exercise their influence on the brain and nerves; and, *secondly*, If we

shew that in no one instance, on any of the gentlemen, who subjected themselves to this operation, did those stimuli evince a selection for the nervous system, as the seat of their action, may we not assert, that we have pointed out, from the influence which they exert on the sanguifeus system, the impropriety, of exhibiting them in those states of disease, in which the blood-vessels are laboring under engorgement.



SECTION II.

Of the Glandular System.

The glandular system next attracts our attention, as much on account of the importance of the functions it exercises in the animaleconomy, as on that of its holding a place among the supporters of the specific doctrine. To different parts of this system, physicians have given to certain stimuli the exclusive right of influence. Thus digitalis and nitre are said to be determined to the kidneys; sulphur and antimony to the skin; and mercury to the salivary glands. But medical writers have omitted to notice either the influence or impression which these medicines exert on the arterial

system, or the irregularity in the results of their exhibition. I am therefore led to relate the following experimental facts, from which I shall beg leave to deduce some arguments, which are opposed to generally-received opinions on the subject.

EXPERIMENT IV.

From the arm of my friend and fellow-graduate, Mr. Morris, I abstracted twelve ounces of blood, with an intention of accumulating his excitability. After this, his pulse beating seventy-six strokes in a minute, I exhibited to him half a grain of powdered digitalis. In five minutes, his pulse beat seventy-eight; in ten, eighty; in fifteen, eighty; in twenty, eighty; in twenty-five, seventy-six; with irregularity in force, he experienced some nausea. The dose being now repeated, in five minutes his pulse beat seventy-six, nausea being removed; in ten, seventy-two; in fifteen, seventy-two; in twenty, seventy-two; in twenty-five, seventy-two: he now took one grain more of the medicine, and in five minutes his pulse beat seventy-two; flushing of face, and sense of heat at stomach now supervened; in ten, seventy, with a sense of tension across the forehead; in fifteen, seventy, pulse very irregular; in twenty, seventy-

two, tension increased ; in twenty-five, seventy-four, more regular ; in thirty, seventy-six, and perfectly regular. I examined Mr. Morris's pulse several times before he left me, without perceiving any alteration in it. I can confidently assert, that our medicine exercised no influence on his kidneys.

EXPERIMENT V.

My friend and fellow-graduate, Mr. W. Washington, his pulse beating eighty strokes in a minute, took one grain of the medicine as above. In five minutes, his pulse beat eighty ; in ten, eighty-four ; his head now became so much affected, as to render it necessary for him to assume a prone position, during which time, I omitted examining his pulse ; in twenty-five minutes, I was surprised to find it had fallen down to seventy-two, nausea now came on ; he then repeated the dose, from which his pulse arose, in five minutes, to seventy-six, and his nausea was dispelled ; in ten, seventy-four, with fulness ; in fifteen, seventy-six, and the fulness increased. In consequence of my now becoming seriously indisposed, my obliging friend was not then further attended to ; but on an ensuing day informed me, that on his return home his head was

much affected, and that our fellow-graduate, Mr. Price, declared that the pupils of his eyes were much dilated. Mr. Washington did not experience any diuretic effect from this drug.

EXPERIMENT VI.

My very obliging friend, Mr. Heap, suffered me to place his system under the action of *conium maculatum*, or hemlock, with the view to ascertain its effects on the pulse. The results were violent arterial action, then depression, and after a repetition of the dose, his heart and arteries were excited to violent re-action. The difficulty of obtaining subjects to place under the operation of mercury, will serve as an apology for the want of experiments with that important article ; but were I to omit saying any thing on the subject of its operation, I should justly merit the censure of my readers. If mercury was a sialagogue, or possessed a specific choice, for the salivary glands, as the primary seat of its action, it would not be presumeable, that physicians, which very often happens, would exhibit, with a view of exciting catharsis, emesis, diuresis, and perspiration, the different preparations of this metal. Calomel we all know to be an admirable purgative medicine. Turbith Mineral,

the professor of materia medica admires as an emetic in many cases; and the oxygenated muriate, when given in small doses, acts as a diuretic, but in larger doses, it induces death. Perspiration has been observed to be the effect of all the preparations. Dr. Barton informs us, in his lectures on the *Materia Medica*, of phagedena having frequently been the consequence of the mercurial medicines when exhibited to children, who had been previously bled. Mercury is a fashionable medicine in amaurosis. Every physician must know, that this disease is confined to the optic nerves, as dissections have long ago demonstrated. Now receiving what has been said above, as self-evident facts, I will ask, can the cure of amaurosis, by mercury, be explained on any other principle, than that of the medicine exercising a primary influence on the arterial system, from which the nerves, as a dependent system, receive an ability to use a concurrence of efforts, by which the deranged nerve is restored to its primitive condition?

SECTION III.

The visceral system* is now introduced into notice. This, on account of its importance as the grand laboratory, and vehicle of our food, ought most indubitably, to attract the serious attention of physiologists and physicians, but not, I think, as an independent system, on account of its resting, for an ability to perform its functions, on the state of the sanguiferous system, which dependence is shewn, *1st*, By that derangement which takes place in this system in different states of disease; *2dly*, By its removal accompanying that of the disease; and, *3dly*, By our curing gastritis with the free use of the lancet, to the exclusion of stomachics, which, with purgatives and astringents, are the articles with which the following experiments are to be conducted:

EXPERIMENT VII.

To my obliging friend, Mr. James Fayssoux, his pulse beating eighty strokes in a minute, I gave, at ten o'clock, twenty

* The stomach and intestines are here only alluded to.

grains of powdered gentian, mixed with syrup. In five minutes, his pulse beat eighty-two; in ten, eighty-four; in fifteen, eighty-four; in twenty, seventy, violent nausea; in twenty-five, sixty-four; sixteen below the natural standard, with an increase of nausea. I now gave him ten grains more of the medicine, with the effect of dispelling his nausea, and re-exciting his heart and arteries, as follows: viz, in five minutes, his pulse beat sixty-six; in ten, sixty-six; in twenty, sixty-eight; in thirty, seventy; in forty, seventy-four; with this gradual increase of action his system was restored to its natural point.

EXPERIMENT VIII.

My friend and fellow-graduate, Mr. M'Donald, his pulse beating seventy strokes in a minute, took half a drachm of pulv rad: columbo, in a spoonful of cold water: in five minutes, his pulse beat seventy-two; in ten, seventy-four; in fifteen, seventy-two, with much force; in twenty, sixty-eight, with a very evident production of hilarity; in twenty-five, sixty-six. The hilarity was so much increased, that Mr. M'Donald declares, was he ever desirous of exciting hilarity, he would swallow a

dose of columbo root, in preference to musk, or any other boasted nervine. In thirty minutes, his pulse beat sixty-four; in thirty-five, sixty-two, with nausea and depression of mind. I now repeated the same quantity: in five minutes, the nausea was removed, his heart and arteries were re-excited to force and frequency, to which hilarity, as before succeeded, he discharged urine twice while with me, which he imputed to the medicine; he now, after two hours, left me, in health, and during a succeeding conversation on the subject, informed me, that the medicine had acted powerfully as a cathartic. Were we to attribute properties correspondent to effects, our medicine, might be classed under the various heads of *incitants, nervines, emetics, tonics, diuretics, and cathartics*.

EXPERIMENT IX.

I exhibited twenty grains of columbo-root to a lady laboring under dyspepsia, with the effect of exciting her sanguiferous system violently, inducing vertigo, turgescence in the vessels of her eyes and head; prostration or suffocation of excitement, and, lastly, vomiting. In this experiment we see, that this medicine was exhibited to its specific system laboring under a specific disease. But here effects very

opposite to those intended, resulted : this medicine, therefore, cannot be said to have been a specific remedy in this case.

EXPERIMENT X.

My obliging friend, Mr. Ravenel, submitted himself to the action of columbo-root, several times, with the effect of having his heart and arteries always highly excited, to which, in most instances, a depressed state of his system succeeded. I did also, on the same gentleman, ascertain by experiment, the effects of *uva ursi*, and *zanthoriza apiifolia*, both native American vegetables, on the pulse. The effect of the *uva ursi* was evident and durable arterial action, while that of the *zanthoriza*, even when exhibited in very large doses, was scarcely perceptible. I am, therefore, led to assert, that the last-mentioned article does not possess virtues equal to those of columbo-root, or gentian.

EXPERIMENT XI.

To Mr. M'Donald, the same gentleman who was the subject of a former experiment, at ten o'clock, his pulse beating seventy strokes in a minute, I gave thirty grains of powdered rhubarb, in a spoonful

of water. In five minutes his pulse beat seventy-two; in ten, seventy-six; in fifteen, eighty; in twenty, seventy-two, attended with nausea. I now repeated the same quantity as before. In five minutes, his pulse beat seventy-six, the nausea removed; in ten, eighty; in fifteen, seventy-two; in twenty, seventy-two; this was the standard of his pulse, until the action was translated to the intestines, which was not until four hours from the time he had taken the medicine. During the time Mr. M'Donald was engaged in evacuating his intestines, his pulse fell to fifty-six strokes in a minute, which could hardly be perceived by the gentleman.

EXPERIMENT XII.

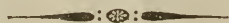
Into the stomach of my friend and fellow-graduate, Mr. Tomas, his pulse beating seventy-two strokes in a minute, I conveyed three grains of powdered galls. In five minutes, his pulse beat seventy-eight, with increased force, and a sense of warmth at stomach: in ten, seventy-eight, fulness of the head and eyes, with slight pain; in fifteen, eighty-four, considerable increase of force, and warmth at stomach; in twenty, seventy-eight, with tension; in twenty-five, seventy-eight; in thirty, seventy-eight; in thirty-five, eighty: the tension diminished, to which a production of hila-

rity succeeded. Having omitted to examine his pulse until ten minutes had elapsed, I was much astonished to find, at forty-five minutes, that it was reduced to seventy-two, with some nausea; in fifty-five, seventy, with increased nausea. The dose was now repeated: in five minutes, his pulse beat seventy-eight, and the nausea removed; in ten, eighty; in fifteen, seventy-eight; in twenty, seventy-four: recommencement of the nausea, dull pain of the head, with depression of mind; in twenty-five, sixty six, he now left me, and the next day informed me, that on his way home he was so severely affected with nausea, as to fear being necessitated to vomit in the streets. He was, in a few hours (he omitted noting how many) purged by this astringent drug.

To my friends, Mr. Logan and Mr. Heap, I gave several doses of our medicine, and with very similar results. I am, therefore, led to infer, that galls, in consequence of the durability of their action, on the heart and arteries, would be (either alone or in combination with the Peruvian bark) an admirable medicine in the cure of the intermittent states of disease, and in all cases of chronic debility.

With alum also have I experimented, and ascertained, that it exerts a powerful

influence upon the sanguiferous system, together with the production of effects similar to those induced by other articles. As to the purgative and emetic effects of this article, I refer the reader to writers on the subject.



SECTION IV.

The Muscular System.

The muscles of our body too, have been formed into an independent system, have had particular diseases assigned to them, and specific remedies for the cure of those diseases. At the head of these remedies stands the celebrated oil of amber, which I, labouring under the fullest influence of this theory, exhibited in a case of tetanus, without effecting the least perceptible remission in the spasmodic paroxysms. The failure of our medicine, in that case, I *then* imputed to the violence of the disease, and I should, in all probability, have persevered in my former opinion, to the end of my life, had I not instituted the following experiment:

EXPERIMENT XIII.

At ten o'clock, my friend and fellow-graduate, Mr. Nelson, his pulse beating eighty strokes in a minute, took twenty drops of oil of amber, on a lump of sugar. In five minutes, his pulse beat eighty-two; in ten, eighty-four; in fifteen, eighty-four, sense of warmth at stomach: in twenty, seventy-six, nausea. He now repeated the dose. In five minutes, his pulse beat eighty; in ten, eighty; in fifteen, seventy-six; in twenty, eighty; and in twenty-five, eighty. It may be worthy of notice, that the pulse of Mr. Nelson, at the commencement of the experiment, beat so feebly as to be scarcely perceptible to Mr. M'Donald, and myself, while now it evinces to each of us some force. Mr. M'Donald experiences some hilarity, but no increase of muscular power. This experiment has convinced me of at least two things, viz. *1st.* The impropriety of forming too sanguine expectations from any theory whatever; and, *2dly,* That too much dependance ought not to be placed on weak remedies for the production of powerful effects.

The sanguiferous system has attracted much attention, and observation, throughout the course of the preceding experi-

ments : not conceiving the juices of the body to form a system, as they depend for their existence on very casual circumstances, they, together with the absorbent system, shall be passed by in silence. During the whole course of this experimental inquiry, I was much astonished and pleased at observing, that on the supervention of any other effect, or concurrence of effects, such as nausea, hilarity, and catharthis, in the experiment with Mr. M. with columbo root ; nausea in that with Mr. Heap, with assafætida, and tension in the forehead of Mr. Morris from digitalis, together with other, and similar effects on some other gentlemen, who were the subjects of experiments, with other articles. The conclusions to be drawn then from the preceding experiments are, 1st. That none of the articles with which they were conducted, appear to act invariably in one way ; but although I do not credit the theory of specific action, yet I acknowledge, that they may be, and are, made to induce some effects oftener than others ; and, 2dly, That effects, often resuted from their exhibition, which were not anticipated. Having now, as I think, proven by experiment, that arterial action is the primary evident result of the application of stimuli to the animal body ; and having demonstrated a diminution of arterial action, to commence with the incipient succession of any other effect

or action, it may not, perhaps, be superfluous to observe, that there does exist a vital, or excitable principle, upon which (it is evident both from experiment and observation), stimuli do exert an influence; but the manner in which they act to work those effects or changes, which result from their application or exhibition, it remains yet for some happy physiologist or physician to unfold. As we cannot, by the aid of the specific doctrine, explain satisfactorily all those phenomena which evince themselves, in the animal body, as the product of stimulative action, may I not be allowed to suppose, that tone in the stomach, the effect of columbo root and gentian; emesis, that of ipecacuanah and sulphate of zinc; catharsis, the effect of jallap and rhubarb; hilarity, that of musk, and assafætida; muscular strength the effect of oil of amber and habitual exercise; ptyalism that of mercury and copper; diuresis, and perspiration the effect of digitalis and antimony; are as much and invariably the effect of a primary action on the heart and arteries, through the medium of the stomach, as sleep, the effect of opium, is that of a primarily excited arterial action? To give some degree of plausability to this apparently, visionary suggestion, I might have recourse to many facts, some of which are afforded by Mr. Maclean, of Calcutta,

which, with others I have collected, are inapplicable on any other principle. Mr. Maclean, in many cases of dysentery exhibited large quantities of opium, without inducing sleep or constipation; but, on the contrary, after having overcome tenesmus and griping, the medicine induced free and frequent alvine evacuations, and in many instances ptyalism, perspiration, and diuresis. From these facts, we may draw the conclusion, that opium is one of those medicines which we may, by a close and judicious attention to the states of the system, so adapt to its excitability, as to induce, at will, either of those effects, which were in the practice of Mr. Maclean, the mere products of accidental circumstances. I am strengthened in this opinion, by the fact of my friend and fellow-graduate Mr. Geddy, becoming, under certain circumstances, purged by doses of this medicine; and under the direction of Mr. Maclean, has salivation, the effect either of mercury or opium, been removed by its judicious and regular administration; and to Mr. Jackson, another friend and fellow-graduate, am I indebted for the fact, that, in consequence of his having, for several weeks past, abstained from pepper, salt, and other high stimulants and condiments, he became, on recurring to their use, severely affected with catharsis.

In the treatment of typhus-fever, Mr. Maclean, has exhibited mercury, and had every effect to result from it, that might have been expected; effects similar to those induced by musk, opium, and tincture of cantharides. He never, in one instance, unless some irregularity or intermission had taken place in its exhibition, had ptyalism or profuse catharsis to occur. The following case may serve to add some strength to the opinions of the Calcutta physician: Mr. ——— laboring under syphilis, placed himself under the operation of mercury, by wearing flannel drawers lined with mercurial ointment; these he repeated every night and morning, with the effect of having his arterial system highly, though regularly, excited; to reduce which, I abstracted twelve ounces of blood from his arm. In twenty four hours after this, he was affected with profuse salivation. In consequence of an increase of the syphilis, I combined small quantities of opium and calomel together, which were exhibited with the effect of removing the ptyalism.

Without extending the principle which I have laid down, I think we may, with some degree of plausibility, explain the solution of fever, on the principle of a translation of action. In support of this theory, I may observe, that I was not, long

since, happily relieved, of an incipient fit of fever, from an effort to vomit, excited by weak tea. A disappearance of all febrile symptoms on the appearance of black vomit in yellow fever, is a fact well known to every physician. That the same effect succeeds an effort, in the cuticular emunctories, to relieve themselves, I convinced myself of, by attending to the termination of a fit of fever, which has just gone off,

To close the preceding pages, without an acknowledgement of gratitude and esteem, to the different Medical Professors of this University, would be acting too repugnantly to my feelings. Accept, then, Gentlemen, my best wishes for your individual happiness; and that each of you may live long in the perfect and uninterrupted enjoyment of those faculties, which you now exercise, with so much credit to yourselves, justice to your pupils, and benefit to mankind, is the unfeigned wish of him who bids you an affectionate, though reluctant, adieu.

NOTE TO PAGE 12.

DR. RUSH says, one of the facts therein contained is mistated, and that, "*The three young men were exposed to the exciting cause of disease from playing in the open air on a spring day—not to contagion.*" The remaining part correct.

FINIS.

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